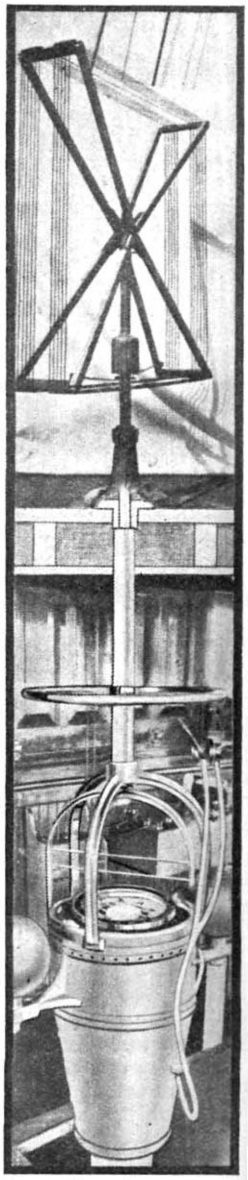
Directive Radio-telegraphy and Navigation.

In foggy weather, sound-signalling stations have proved useful as an aid to navigation. The sounds heard, however, cannot be trusted to give accurate indications either of the distance or direction of the station. Their range also is very limited. It is not surprising, therefore, that many suggestions have been made for utilising the electric waves used in radio-telegraphy to enable a navigator to find his bearings. The propagation of electric waves is unaffected by fog and, unlike sound or light waves, can be transmitted to any distance. Moreover, the apparatus required for radio-signalling is very cheap, requires little skilled attention, and can easily be installed in lighthouses and lightships. Until two or three years ago the radiophares—or radio-beacons as they are called in America—were purely stations for giving ships their positions. In order to find its bearings a ship must send a message to two or more stations, and its direction is located by direction-finding coils. The stations then communicate with one another and so, by the help of triangulation, find the position of the ship, which is communicated to it by radio-telegraphy. In practice the whole operation takes about five minutes. The most extensive chain of direction-finding stations is that controlled by the United States Navy. There are at least thirty stations on the Atlantic seaboard and several on the Pacific coast. France has about ten radiophares and this country has six. A drawback to the method is that valuable time may be lost in getting into communication with the radiophares and in getting the information back to the ship.

A new and very promising method has been recently developed by the Bureau of Standards at Washington in co-operation with the Bureau of Lighthouses. In this method lighthouses and lightships the locations of which are accurately shown on sailing charts are equipped with radio fog-signalling apparatus. A direction-finder operated by the navigating officer is installed in the ship. It is then easy to find the directions of the various radiophares within his range and thus work out his position on the chart. The results obtained by this method have been very successful, and it seems to be much preferable to the ordinary method of using the direction-finder at the fixed stations. It seems probable that every important lighthouse in America will soon become a radio fog-signalling station. The Bureau of Standards suggests that radiophares should be divided into three classes. The first, or long-range class, has a radius of action of 300 miles. The second, or short-range class, can signal to 30 miles; and the third class comprises the lightship stations which can signal to 10 miles.

The method has been made possible by the perfecting of a new radio direction-finder. The principle on which it acts is that the signals received by a flat coil have their maximum intensity when the direction from which they come is in the plane of the coil. When certain precautions are also taken in arranging the apparatus, the signals are of practically zero intensity when the plane of the coil is perpendicular to the direction from which they come. As Fig. 1 shows, it is designed to be installed over the ship's binnacle carrying the magnetic compass. The radio-bearings are read directly on the magnetic compass card. An additional scale, marked with the corrections obtained when calibrating the instrument, is attached to the top of the binnacle so that the true reading can be obtained at once. When taking a bearing the only operation necessary is to rotate the direction-finding coil until the sound is a minimum. The ordinary type of direction-finder for use on shipboard consists of a coil of ten turns of insulated copper wire wound on a wooden frame four foot square, which is mounted so that it can be rotated about a vertical axis. Suitable receiving apparatus is used in connection with this coil, namely, a variable air condenser for tuning purposes, a six-tube amplifier having three stages of radio-frequency amplification, a detector, two stages of audio-frequency amplification, batteries, and suitable telephone receivers.



direction-finder attachment for reading the radio-bearing directly on the magnetic compass.

The Bureau of Standards have issued a pamphlet by F. A. Kolster and F. W. Dunmore giving a full description of the direction-finder, pointing out some of the difficulties that had to be overcome in developing it, and giving many experimental results.

The Cause and Character of Earthquakes.[1]

By R. D. Oldham, F.R.S.

The study of earthquakes, using that word in the restricted, and original, sense of the disturbance of the ground which is sensible to human feelings, which causes alarm and destruction, and is properly that seism of the ancient Greeks, from which our modern term seismology is derived, has always been recognised as one of the departments of geology. This limitation is necessary, for, of late years, seismology has been extended to the study of a phenomenon of different character, the long-distance records of disturbances, only to be detected by very sensitive instruments of special construction; in some cases these are clearly connected with great earthquakes—as the word is here used—and by inference have been presumed to be so in all cases, even when there is no independent evidence of the earthquake proper. The records, regarded as records of the progressive enfeeblement of the larger disturbance of the true earthquake, would represent the cryptoseism, or unfelt earthquake, and be described correctly in the observatory records as earthquakes. That they are correctly so described is indisputable, if the word is taken in its literal interpretation as a quaking, however feeble, of the earth; but if the implication is added that they have the same origin as the greater disturbance, the correctness of the description becomes doubtful.

---

[1] Abridged from the presidential address delivered before the Geological Society of London on February 17.

Some dozen years ago the results of a study of the records of the Californian earthquake of 1906 led me to point out that, while the immediate origin of the earthquake proper may be traced to occurrences which take place in the outermost parts of the earth's crust, these are but the secondary result of a deep-seated origin, or bathyseism, which gives rise, at the same time, to the disturbance which is recorded at long distances by suitable instruments. Later work and research has more and more confirmed both the correctness of this interpretation and the conclusion that the proximate cause, of great and destructive earthquakes, is distinct from that of the long-distance records, though the two origins are connected with each other as effect and cause.

In the present state of our ignorance of the nature of the bathyseism, it is difficult to give a clear and precise definition of the mode of connection between it and the earthquake proper; the subject is an interesting one, and a review of the evidence, together with the deductions which can be drawn from it, would fill the time available, but it is not my intention to do more than to attempt, by analogy, to illustrate and explain the nature of the connection of the bathyseism with its two independent results.

Not many years have passed since, in the south-eastern corner of England, we heard what were known as the guns of Flanders; and the description was correct. The sound—it was more a sensation than a sound—which was heard in Kent and Sussex was undoubtedly produced by the report of great guns, by the explosion, that is, of the charge in the gun itself; but had the explosion done no more than give rise to the sound waves which travelled far in every direction it would have little troubled the enemy. Simultaneously, however, with the production of the report, and by the same explosion, a projectile was sent flying through the air which exploded after a trajectory of some miles, causing the damage which was the purpose of its despatch. The effect of this second explosion was severe but local, and at a short distance away neither sound nor shock was sensible.

Here we have a very complete analogy; the explosion of the gun represents the bathyseism; the report and sound waves travelling afar, correspond to the disturbance which, propagated through the substance of the earth, gives rise to the long-distance records: the explosion of the shell to those dislocations in the outer crust which produce the destructive earthquake; and the trajectory to the connection, of which the character is as yet unknown, between the bathyseism and the surface shock.

If this interpretation be accepted, it becomes evident that the distant records represent something which is distinct from the earthquake, as originally understood, and that the study of records, with the deductions drawn from that study, have little or no bearing on the problems of geology, as we usually limit the scope of that science. It is otherwise with the earthquake proper; originating in, and affecting, the outermost crust of the earth, it has long, and rightly, been regarded as one of the departments of geology, both as regards cause and character, and it is with this aspect of the subject alone that I shall deal.

The character of earthquakes is known to an extent sufficient for my purpose; they are elastic waves, transmitted through the substance of the earth, not, as was once supposed, merely waves of elastic compression, but of most complicated character, and, in all but a small minority of cases, nothing but this vibratory movement, the orchesis, can be recognised. Occasionally, however, and only in the case of some earthquakes of destructive violence, there is also a bodily and permanent displacement of the solid ground, and this mass, or molar, movement has been distinguished as the mochleusis of the earthquake, as distinct from the elastic displacement, accompanied by return to the original position, which constitutes the orchesis. Now the elastic waves can only be initiated by some sudden impulse or disturbance, such as might be produced by the fracture of rock, and as, in those earthquakes where mochleusis can be recognised, there is usually evidence of sudden movement along some pre-existent fault-plane, or of rending and fissuring of the solid rock, faulting or fracturing has come to be regarded as the cause from which the vibratory disturbance, propagated through the unfractured rock, originates.

This conclusion is supported by the fact that the proximate origin of the shock can almost always be placed at a moderate depth from the surface. It is, unfortunately, impossible to give any precise figures, for none of the methods which have been suggested for determining the depth of the origin can be trusted, some because they depend on assumptions which the progress of knowledge has shown to be erroneous, others because they demand data which cannot be supplied with the requisite precision, if at all; but there is another way in which some idea of the depth of origin may be reached, based on the fact that there is usually a well-defined area of maximum intensity of shock, surrounded by regions of diminishing intensity, as the distance from the central area increases. Since the violence of the disturbance will decrease with the increase of distance from the origin, it follows that, the nearer the origin lies to the surface the more closely does the variation of surface distance from the epicentre approximate to the variation in actual distance from the origin; hence it is evident that the rate of variation of intensity of the disturbance will give some notion of the depth of the origin. In this way, quite apart from any numerical estimates which have been made, it becomes clear that, excluding a small minority of earthquakes which will be referred to later, the origin lies at a very moderate depth below the surface, probably seldom over ten miles, and usually less, that is to say, within the limits of the solid outer crust of the earth; and in this region it is difficult to conceive of any cause, sufficient to originate the elastic wave-motion of the earthquake, other than the sudden fracture of the solid rock, where strain has outgrown the power of resistance.

Apart from this general reasoning from observation, there are cases on record where considerable displacements of the ground have been measured by the comparison of careful and accurate surveys made before and after the earthquake. In three of these—the Cutch earthquake of 1819, the Sumatran of 1892, and the Californian of 1906—the largest movements took place close to the line of fracture, and in opposite directions on opposite sides of it, the displacements decreasing on either side till a region was reached in which no change, from the condition before the earthquake, could be recognised. As this is precisely what would take place if a solid body, capable of elastic deformation, was strained until fracture took place, the conclusion is justifiable that such was in fact the origin of the dislocation and displacements.

So far the conclusions, which may be drawn from observation, as they have been briefly outlined, belong rather to the domain of physics than of geology, but when we go on to consider the cause to which the strain is to be attributed, and more especially the rate of growth, we are brought into contact with problems and deductions which are intimately connected with geology proper, and to which I propose to confine attention in the remainder of this address. As regards cause: this is usually attributed to what are known as the tectonic processes, a term which may approximately be described as the processes by which the folding and faulting of rocks were produced, and, in accordance with this attribution, the class of earthquakes with which we are concerned is referred to as tectonic. The rate of growth of strain has almost invariably been accepted as very slow, yet when the subject is looked into, it will be found that there is really no evidence to support the acceptance; in part it must be attributed to the general belief that all geological action is necessarily slow, and in part to the conclusion that the Earth is a solid inert and highly heated body, cooling slowly by radiation, with the subsidiary deduction that all deformation of the outer crust must be referred to contraction, consequent on that slow cooling. The latter of these reasons is now abandoned by those who forced it on us, and the former, though true in general, must not be treated as an unchangeable law, for there are many cases where a process, slow on the average, and as a rule, is occasionally subject to temporary acceleration of rate. The evidence, too, which has been regarded as confirmatory of the slow growth of strain, is, in truth, more properly described as an interpretation of observed facts in accordance with an hypothesis.

In the report on the Californian earthquake of 1906, for instance, the displacements caused by that earthquake and an earlier one in 1868 are explained by a slow growth of strain, extending over a century or so, partly relieved by fracture in 1868, and again in 1906. The argument is conclusive in so far as it shows that the effects are consistent with the hypothesis, but it was not noticed that they would be equally consistent with a condition of quiescence throughout the whole period, with the exception of two short intervals immediately preceding the two shocks, respectively. The same may be said of all the supposed evidence in favour of a slow growth of strain; it is true that in those earthquakes which have been investigated in detail, and in which the observations allow of any definite conclusion, the indications point to the conclusion that the proximate cause is fracture resulting from excessive strain, but there is in no case any evidence of the rate at which that strain accumulated; nor is it possible that any such evidence could be found. The after-effects may satisfactorily establish the conclusion as to the cause, but they can give no indication of the time occupied in preparation; the earthquake comes and passes, it leaves certain records behind it, but these records would be the same whether the preparatory growth of strain was secular or instantaneous in duration.

Yet the problem is not insoluble, for there is another line of attack, which has only become practicable within the last few years. If we regard the growth of strain as continuous, there will be a certain increment which will lead to fracture, earthquake, and partial relief; then with a further increment the process will be repeated, and so we reach the concept of a mean-strain interval for each shock, which may be regarded as constant, on the average, for any given region, provided that the average is taken over a sufficiently long period. Any variation in the rate of growth of strain must be accompanied by a corresponding variation in the frequency of earthquakes. We have, then, four quantities so related to each other that if three of them are known the fourth can be determined. Of these four, two, namely the mean frequency and the variation from that mean in any chosen period, can be obtained from observation, and if the variation from the mean rate of growth of strain is also known, for the selected portion of the whole period, that mean rate which is the object of search can be obtained by a simple rule of three sum. So that if there were any external cause which, acting periodically and alternately in increase and decrease of the rate of growth of strain, and if it were possible to disentangle the variations due to this from those due to other causes, we would have a means of framing a numerical estimate of the general rate of growth of strain.

One such cause of periodic variation is to be found in the tide-producing stresses set up by the sun and the moon. It is true that many attempts have been made at different times to detect some connection between the frequency of earthquakes and the position of the moon, and that no such connection has yet been established, but these attempts have all been based on very imperfect records. In time it may, perhaps, be possible to apply to an earthquake record the method of harmonic analysis, which has proved so fertile in the case of the ocean tides, but the day is long distant when a record of sufficient completeness will be available. Meanwhile there are some simpler relations, of which a discussion is feasible, and the most promising of these seems to depend on the fact that the downward pressure is greatest at the time when the attracting body is on the horizon, and least when it is on the meridian. If, then, we divide an earthquake record into two groups, one containing all shocks which occur within six hours before a meridian passage, and the other all that happened within six hours after, one of the two groups will cover a period during which the downward pressure is, on the average, increasing, while the other will cover the period during which it is decreasing. As the amount of the change so introduced is known, with sufficient accuracy for the present purpose, and as it should, on the hypothesis being used, influence the frequency of earthquakes, it follows that we have here a method, which should enable us to make an estimate of the rate of growth of the strain, to which fracture is due.

Although simple in principle, the method is difficult in application. To begin with, a record is required of sufficient extent and continuity to give a trustworthy average, not merely of the general frequency, but also of the frequency in each of the two sections into which it is divided, and this in practice means that the record must contain at least two thousand shocks and ought to contain double that number or more. Then it must be reasonably accurate as to times and complete as to occurrences, or at least must be fairly uniform in its incompleteness over the whole period investigated. There are not many records which fulfil these primary requirements, but there is another even more important. In all records there is a noticeable variation in frequency at different times of the day, moreover, the nature of this diurnal variation has been found to vary in different regions, but appears to be constant and characteristic, in each region, over the period of record. The cause of this periodicity may reasonably be attributed to some effect, meteorological or other, connected with the daily course of the sun, but its nature, no less than its variability, shows that it can only be attributed in part, if at all, to gravitational attraction. It is only, therefore, by a conversion of the record from solar to lunar times that the influences of these other effects can be eliminated, and the gravitational attraction of the moon be detected and estimated, and, for the satisfactory application of this method, it is necessary that the record should cover a complete lunar cycle, or a period of nineteen years. There are only two records extant and available which fulfil this requirement, and of these the Italian is not only the most complete and accurate, but is the only one to which the conversion into lunar times has been applied.

From the summary of the figures obtained, published in our Quarterly Journal, it appears that in the six hours preceding and following a meridian passage the mean departure, from the general average for six lunar hours, is almost exactly 1 per cent. of the mean.

Passing over details of calculation, the average rate of growth of strain is found to be such that the breaking point would be reached in about two months from the start, with a wide variation on either side. Some other relations between the frequency of earthquakes and the diurnal variation of the tidal stresses might be, and have been, investigated; all give fairly confirmatory results, the longest period indicated as required for reaching the breaking strain being just about a year.

It must not be supposed that value can be attached to the precise figures. As is invariably the case, in all calculations regarding physics of the earth, many considerations are involved of a very uncertain nature, but the reasoning does show that the increase of strain must have taken place at such rate that the breaking point was reached in a period measurable at most by months, and shows that the period could not have been of such length as to be measurable by years or decades, for, had this been the case, the disparity dealt with would have been much greater than that actually found.

(*To be continued.*)

Obituary.

PROF. G. S. BOULGER.

BY the death of Prof. G. S. Boulger on May 4, botanical science has lost an accurate and advanced observer who did much to popularise the study of his favourite science, and left his impress on several generations of devoted students. He was an active botanist to the end. On April 26 he attended, as chairman, the meeting of the Botanical Section of the South-eastern Union of Scientific Societies, and he himself was struck by the difficulty in breathing which he experienced in mounting a number of stairs to the meeting-room. Almost his last words on his death-bed had reference to the preparation of the report to be presented to the Congress of the Union at Southampton in June. He died literally in harness.

Prof. Boulger was born in 1853, and was educated at Wellington and Epsom Colleges; at an early age he became Professor of Natural History at Cirencester College, holding the chair for thirty years. Among other appointments which he held were those of professor of geology and botany at the City of London College, and in recent years he was a guide at the Imperial Institute. But his students were a wider public than institutions afforded. He was in considerable request as a public lecturer, and frequently visited, as such, various local natural history societies. He was closely connected with the Selborne Society, of which he was a vice-president, and the magazine of which, *Nature Notes*, he edited for some years. In recent years he was much interested in what has come to be known as Regional Survey, and in this connection did good work for the Gilbert White Fellowship, an up-to-date survey of Selborne being now in progress.

As an author Prof. Boulger had an attractive style. For some time he edited the Proceedings of the Geologists' Association. His knowledge of geology enabled him to throw considerable light on the origin of the British flora. His "Biographical Index of British and Irish Botanists," with Mr. J. Britten, is a valuable book of reference. His "Familiar Trees,"